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GENERAL INTELLIGENCE AND THE PROBLEM OF DISCIPLINE[1]

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Education is believed by effective teachers to be "partly a matter of training and partly of information." Since it is believed in influential quarters that one subject is quite as good as another for training, let us consider for a moment some subject selected and taught for its value as information alone.

It is taught, let us say, in the fourth and fifth grades and again with added subject-matter in the seventh and eighth grades. Perhaps it is later reviewed in the high school. Yet no matter how thoroughly it is taught, in a few years the information is almost completely forgotten if its possessor has had no need to recur to it.

There is nothing strange about this. It is the very economy of the mind that it loses information which is not frequently used. The memory is not a filing-case, and if it were, we should find it necessary to remove its useless contents from time to time very much as the memory drops unused items of information. Certain *early* acquisitions of the memory remain permanent. All other information is continually being replaced by that learned later. This economy of the mind in forgetting furnishes the basis for much

[1] This paper was read at the Nashville meeting of the Classical Association of the Middle West and South.

The books and articles chiefly consulted and referred to in the preparation of this paper are: William Brown, *Mental Measurement*, pp. 132–33, 98–127; Cyril Burt, "Experimental Tests of General Intelligence," *Brit. Jour. of Psych.*, III (1909–10), 95–177; Bernard Hart and C. Spearman, "General Ability, Its Existence and Nature," *Brit. Jour. of Psych.*, V (1912–13), 51–84; G. E. Mueller, "Neue Versuche mit Rueckle," *Zeit. f. Psych. u. Physiol. d. Sinn.*, LXVII (1913), Hefte 3 u. 4; W. G. Sleight, "Memory and Formal Training," *Brit. Jour. of Psych.*, IV (1911), 386–457; C. Spearman, "The Theory of Two Factors," *Psych. Rev.*, XXI (1914), pp. 101–15; E. L. Thorndike, *Educational Psychology*, ed. 1903, pp. 29, 30, 34; ed. 1910, pp. 187–88; W. H. Winch, *When Should a Child Begin School?* pp. 3–6; *idem*, "The Transfer of Improvement of Memory in School Children," *Brit. Jour. of Psych.*, II (1906–08), 284–93; *idem*, "The Transfer of Improvement of Memory in School Children," *ibid.*, III (1909–10), 386–405; *idem*, "Some Relations between Substance Memory and Productive Imagination in School Children," *ibid.*, IV (1911), 95–125.

criticism of schools by persons who suppose that information is the sole aim of study. If it *were* the sole aim, I think myself that study and teaching would result in rather dismal failure.

Observing this rapid loss of facts and noting also that an educated man is somehow vastly more efficient than an uneducated man; noting also that the educated man always has at his disposal a great store of *new* information, all this, I think, led to the opinion of competent teachers that the act and practice of learning might be of more value than the information acquired, that the learning habit might be usefully applied after the data by means of which the habit was formed had been forgotten.

This is the pernicious and derided doctrine of discipline. There *is* no learning habit, it is replied. If you have a scholarly habit of crossing your *t*'s, it will not lead you also to dot your *i*'s. Crossing out *A*'s on a page of printed letters has only 52 per cent of relation to crossing *a*'s and *t*'s. Since the *a*'s make up 50 per cent of the work, perhaps the crossing out of *t*'s is a mental function almost totally independent of the function of crossing out *a*'s.

Such statistical results led at once to an atomistic conception of the mind. There is no faculty of perception, but a faculty or function of perceiving *a*'s and another faculty of perceiving *t*'s, etc., i.e., these functions are as numerous as the objective data to which they are applied. But let me quote.

> There are no *few* elemental faculties or powers which pervade each a great number of mental traits so as to relate them closely together, ... the mind must be regarded not as a functional unit nor even as a collection of a few general faculties which work irrespective of particular material, but rather as a *multitude* of functions each of which is related closely to only a few of its fellows, to others with greater and greater degrees of remoteness and to many to so slight a degree as eludes measurement. . . . . The science of education should at once rid itself of its conception of the mind as a sort of machine different parts of which sense, perceive, discriminate, imagine, remember, conceive, associate, reason about, desire, choose, form habits, attend to. Such a conception was adapted to the uses of writers of books on general method and arguments for formal discipline, and barren descriptive psychologies, but such a mind nowhere exists [p. 29].

Forthwith the official science of education did rid itself of the old conception of mind and became entirely nebulous. But one

thing was apparently agreed upon, namely, that the word "training" should no longer be used in lectures on education nor be used in its definition.

This atomistic conception of the mind carries with it a new conception of the school. The school is no longer a place where the teacher contributes anything to the future welfare of his pupils. It is instead a selecting agency to pick out the bright pupils and eliminate all industrious mediocrity. The *successful* teacher is then the one who can most quickly convince the plodder that he must not aspire to scholarship. This turns the schools over to an entirely new type of persons as teachers. I confess that this conception of the school as an instrument of unnatural selection has always been distasteful to me and seemed a trifle too aristocratic for our democratic country.

We now have a right to ask whether this banishment of the idea of training from education rests on a thoroughly scientific basis, and whether any other interpretation of the facts is possible or probable.

The facts to be explained first are not derived from experiments which involved training. Those must be mentioned later. Since the Pearson formula showed little or no correlation between the results of the mental tests used, mental functions, it was concluded, are numerous and almost completely independent of each other. Recently the English mathematical psychologists have taken up the problem and they have refined the mathematical formula to be applied. To be brief, Professor Spearman, of University College, London, has taken the data already shown by the less refined method to give little or no correlation, and finds 91 per cent of correlation. From such evidence he concludes "that all the intellective activity of any person depends in some degree on one and the same general fund of mental energy"—one of the oldest, most widely held hypotheses of psychology.[1]

This is of course equivalent to the old view of general intelligence and is flatly opposed to the conception of mind as composed only of a multitude of specific, independent functions.

The result is that by this method of studying the mind we have reached a flat contradiction and dispute. It may require years to

[1] *Psych. Rev.*, p. 103.

decide which conclusion is the true one, and I for one dislike to give up the evidence of experience until experiment can make a better showing. The early results of correlation experiments so positively contradicted ordinary observation and experience that I could never persuade myself that the method is infallible, as it was assumed to be. If Professor Spearman is correct in his results the early work attained just the opposite of the truth.

But worse remains behind. Another investigator points out that "a correlation coefficient is only a statement of probabilities. It does not prove *anything*" (Brown, p. 132). "For the production of the hypothesis itself we must look elsewhere, viz., to psychological analysis and psychological insight" (Brown, p. 133).

We are then in the predicament of having changed our views of the mind and of education on very flimsy evidence, or perhaps on no evidence. Because the so-called proofs were given mathematical form we surrendered at discretion. Now the mathematical results seem to confirm the belief in general intelligence, of which belief we have just "rid ourselves." *Just as much* as the early computation was urged to show that "the effect of training is not transferred," the later computation may be adduced to show just the opposite. Neither showing would be valuable, I think, unless it were sustained by experiments designed to investigate the effects of training directly. Let us turn to these.

Long before the indirect, correlation method was devised, Professor James had found, after training himself by learning the first book of *Paradise Lost*, that he did not learn 158 lines of another poem as quickly as he had another 158 lines of the same poem before training. Two of his students did the same experiment and made considerable gain. Two others made no gain. Professor James himself admitted that "there was some question of the validity of the second test," because he had been considerably fatigued by other work. Though the results were surely inconclusive, the conclusion was drawn that one's native retentiveness is unchangeable. Since that time numerous more careful experiments have been made with just the opposite results. I referred to many of these experiments in the paper written for my colleague Dr. Norlin. I wish to

refer to one more, though it is an extreme case of the number-memory of a lightning calculator. The period of training, four years, was so long that very definite results appeared. Before training he could recite 25–36 figures which were read to him once; after training, 60 figures. The time required to learn 204 figures decreased to almost half the former time. In general, four years' training had almost doubled the grasp of his memory for numbers and diminished by one-half the time required for learning them. The effects of training were therefore enormous. Something much like this probably occurs in high-school students of Latin who cannot read ten lines at the opening of the year, but who read five or six pages at its close. Number-memory then is susceptible of change to an enormous degree by training.

But has such training both a direct and an indirect effect? Does the effect of training transfer or spread in some measure from the activity trained to related ones? Yes. It has been shown that learning 74 or 59 or 100 lines of poetry improved the ability to learn prose among the children tested. Fifty minutes of rote memory, for meaningless data, namely letters, distributed to each of three days, resulted in an improvement in substance-memory of 11 per cent, in experiments which seem to me very carefully planned and executed. In these experiments there was almost as much improvement transferred as was made in rote memory itself, which was directly practiced. This of course was exceptional. A small transfer, however, was uniformly present.

In other words, rote memorizing of *meaningless* data, such as letters, does help logical or substance memory to some extent. This transfer is suprising. I should have guessed, for example, that the severe training of verbal memory required in Latin and Greek would work prejudicially on substance memory, i.e., that verbal memory would take energy from substance memory and diminish its efficiency. The experimental results I have mentioned are evidence against such a guess.

Extremely long and *severe* memory training on numbers did diminish the efficiency of immediate memory for color names, for consonants, and for nonsense syllables, if we return to the tests of the memory of our mathematical prodigy once more. His training

continued for eight hours per day for four years. Few students will devote themselves so steadfastly to study.

However, this very fact that very intense and prolonged exercise of one mental activity seems to diminish the efficiency of others shows that the mind is not a lot of independent functions each of which must receive specific training if it is to be developed at all, as has been claimed with such tremendous authority. Instead, training in one type of memory immediately begins to benefit other types to a greater or less extent, exactly as competent teachers have long believed, and too prolonged training of one type appears to take energy from other types. This does not look like independence but like a common faculty at the basis of the several types. I do not mention memory because of its great importance, but because there are now ever so many careful studies of the effect of *training* the memory.

There is also one excellent experiment to test the effect on *imagination* of training in substance memory. "Improved memory resulted in improved work in imagination," notwithstanding we have always supposed that the routine of school crushed spontaneous fancy. When, however, training was carried to the point of fatiguing the memory, the imagination was also fatigued, or at least the practiced children then did worse than the unpracticed group in imagination. This fact throws much light on Professor James's early experiment. Since all these experiments on the effect of training were carried out on children, allowance was made for improvement due to growth.

Another investigator, Sleight, got similar results, but his mathematical treatment of them reduces their numerical amount in *some cases* but not in all. The major portion of his paper is devoted to computing away and explaining away the results of the experiments.

Winch's work, which I have quoted, seems to me the better. There will doubtless be discussion for a long time to come, but the independent-function notion is being tacitly abandoned, and experimental pedagogy is confirming more and more the observations of conscientious teachers. It is promising indeed to see educational psychology cease to dictate laws to nature and proceed instead to discover such laws. The dictation method was, however, always

a rather local affair. It was taken too seriously by official educators. That is all.

Almost without exception, therefore, when the effects of training have been studied by means of training, transfer of its effects have resulted. Correlational psychology seems to point also to general intelligence, though its evidence is not so certain as that of training experiments.

The psychologist Meumann is probably the greatest authority on experimental pedagogy. His conclusions may therefore be of interest.

> It is always shown that the formal value of school practice is only moderate. If you subject a normally gifted child to artificial practice, his powers increase considerably in every respect. From such experimental results the conclusion is probably to be drawn that we must return to the principle of formal training in the *Volksschule*. . . . . Especially the Belgian educator Von Biervliet has made demand to introduce the *purely* formal exercise of memory, perception, observation, and judgment into the *Volksschule* in order to increase the development of capability in the school child" [I, 466].

But Van Biervliet means training on *completely senseless* material. So Meumann objects to this but says, "I believe that we should do better to demand that school instruction use the given subject-matter more for formal discipline" (I, 467). Elsewhere (Baird, p. 350), he says, "It is absolutely necessary to introduce into the schools a formal training of the memory such as we have suggested. . . . . The pupils' *purpose* should be awakened to the importance of the education of memory as such" (I, 469). These opinions of Meumann's are based on the most careful and extensive experiments on school children. Yet these opinions sound strangely like the despised doctrine of formal training which was held in America years ago and then pronounced a heresy by official educators. To this day perhaps no one in America would dare to hold such an opinion, yet it is interesting to translate it from this great authority who has been studying the pupils of schools by no means so surrendered to the doctrine of interest as are ours. Nevertheless an opinion quite similar to Meumann's has been held unflinchingly for some years by Professors Grandgent and Wendell. Thus the science and the art of teaching are coming to agree at last. Soon some further proof of a psychological doctrine will be demanded

than that it merely contradicts common-sense. I welcome the day, for it shows that our science will be useful. The experiments I have quoted show that there is both direct and indirect training of our powers, and the word "training" may again become a part of the definition and aim of education.

Relative to Latin and Greek, I do not think that their training value depends altogether or even largely on indirect or transferred training. I do not think it has been proved that there is one verbal memory for Greek words and still another for German, English, and Italian, unless one of these languages is taught visually, the other orally, by an appeal to the ear. Instead, it seems to me that I acquire German and French words by a memory and an interest which I developed by the study of Latin and Greek. Of course this means that I merely *read* these languages, but that is precisely what I wish to do with them.

Again, syntax seems to me a very general science and I seem to use the same power of discriminating meanings, relations, and constructions in reading German that I acquired in reading Latin. The parts of speech and their relations then seem so similar that I can imagine one instantly perceiving a relation and yet being quite unable to tell whether he first learned that relation from a German or a Latin grammar. Thinking the relations between the meanings of words seems to me almost a single process, though the words may be new and strange. Outside of languages, moreover, the number of relations seems to me so much smaller than the number of terms to be related that several realms of abstract thinking could be taught by practice in one. I cannot conceive, therefore, that the training value of Latin and Greek is altogether indirect.

All the experimental work which I have read shows more and more relationship between types of mental activity the more it approaches the field of relational and abstract thinking and the more it leaves simple sensory reactions and simple motor performances. This again is one of the arguments for a common fund of energy or general intelligence.

Should it prove that the memory and interest and discrimination acquired in the study of one language are used in the mastery of another, the matter is of importance, for Professor Wendell remarks

that the teaching of modern languages in America has failed. Yet they must somehow be taught, for it is now a common remark that "Volapük and Esperanto are the only dead languages."

At any rate, there is no experimental evidence that I can accept against this introspective belief of mine. Endowing a man with as many memories as there are different stuffs to be remembered depends only on the atomistic conception of the mind, and that is not established by any means.

True, the man on the street will say he has a poor memory for names, or for numbers, etc. But he is merely naming what he is careless about. Make his *living* depend on his memory of names, or of faces, or of signatures and he will surprise you with the suddenness of the development of his memory for these data. Men who have changed their occupations testify to this prompt change in the facts they remember. It is convenient to use the terms "memory-for-names," "number-memory," etc., to name the subject-matter remembered, but these terms do not show that there are all these different types of verbal memory.

While I have now claimed that the statistical psychology of correlation is as yet only a controversy and that, therefore, the atomistic conception of the mind based upon it is not established, you might readily be confronted by a psychologist who would retort that this is merely my opinion, and valueless just because it is not the opposite view. Consequently I ought to support my assertion by reference to a great authority. Professor Binet says: "If ever a question was under controversy it is that of the value of correlations. Two absolutely contradictory opinions are submitted and both claim the force of proofs." Then he cites Thorndike as one for whom "the mind is only an absolutely heterogeneous collection of faculties which are juxtaposed but remain rigorously independent" (p. 51, quoted by Hart and Spearman, *Les Idées modernes sur les enfants*, 1909, p. 242.

Quite undisturbed, therefore, by the controversy, Binet invented intelligence tests which have swept over the civilized world and have been regarded as one of the greatest achievements of applied psychology, the famous Binet scale. Each test consists of four or five little questions for each age, and the result is surprisingly accurate

and valid. Now if the mind is a "host" or "multitude" of minute independent functions, it is difficult to see how a child's intelligence could be determined by five or ten or fifteen questions. Yet it is commonly admitted that the Binet scale does grade intelligences.

I have begged your attention to all this because our teaching, in its methods and in its subject-matter, is influenced enormously by our conception of the mind. Even a pupil's study is influenced by his conception of his mind and its activities. Experimental pedagogy finds that the effect of purpose, zeal, or good will is enormous in learning and in schools. Whether we retain well what we have learned depends in large measure on whether we learned it with a purpose to retain it. It would then be important if we could honestly ask students to study for the very purpose of training. This I think we have a right to do. Students will not give up the training purpose. The auditorium of a certain high school of my acquaintance has its walls lined with banners won in track meets, in basket-ball, etc. This indicates whither the pupils' training purpose is directed. Physical education is fortunate in having a monopoly of this aim.

But if purpose is of prime importance to the success of the pupil it is much more important to the success of the teacher, and I came here chiefly to say that now at last educational psychology contains nothing proved to force conscientious teachers to believe their work is useless and their efforts hopeless, even though they be teachers of the classics. Those subjects have not lost their power to enable a youth who studies them the better to use his wits, the more readily to detect relations, and the more habitually to think honestly and accurately. Because they are difficult these subjects do, I still believe, develop the power of voluntary attention, and because they are definite I think they lead the pupil to despise guess-work and charlatanism, because they will not lend themselves to guess-work. At present the case claimed against these subjects has not been proved. It may be proved tomorrow, but it is more likely to require twenty years of patient experimentation, and the indications at present are that the verdict will be for and not against the usefulness of these studies. Experimental psychology and teaching experience never promised so much as today to come into agreement.

The chief difficulty with experiments to determine the value of any study lies in the fact that pupils' powers are being trained by all their studies. It is, therefore, very difficult to eliminate the influence of activities with which the experiment does not deal. Assuming, however, that this can be done, the method of Winch seems most promising and most reliable.

Suppose we wish to know whether the verbal memory training required in Latin does not damage substance-memory for history. Winch's method could be applied as follows:

Test a large group of students in a history assignment, or on three history assignments. Then divide this large group into two groups equal in ability as shown by the test exercises in history. Let one of these groups spend an hour a day for two or three weeks in the study of Latin, the other an equal time distributed in the same way on some more remote subject, as physics. Then test both groups once more on history. If the Latin group had not worked to the fatigue point and yet did poorer work than at first, we might decide that verbal memory for Latin antagonizes logical memory for history. If the physics group did better than at first and better than the Latin group, that study would seem to serve logical memory for history (if other studies had not brought about the change), because we are dealing with groups, not with individuals. If this condition appeared in all groups for numerous experiments there would finally be no gainsaying the result. Such agreement could be obtained by the expenditure of care and pains and patience.

A more interesting experiment, I think, could be made as follows: Select two groups of students each of equal ability as shown by their marks in three studies, say physics, chemistry, and physiography for one group, physics, chemistry, and Latin for the other. Then ask the members of each group to interpret a knotty contract, an abstract of title, a law, and the description of a railroad's condition as shown by a manual of statistics. An equal amount of time should be given for both groups. The result will be better if all the work is done individually.

My own opinion is that Latin would not suffer even if law students were included in the other group.

A similar test might be carried out with some difficult diagram as the test object. Apparently hundreds of students cannot understand the structure of the ear, either because they have not sufficient visual imagination or because they cannot get the meanings of the phrases which describe the diagrams—I think the latter. Now two otherwise equal groups, one from the class in manual training, the other from the Latin class, might be compared with some such difficult interpretation of meaning of the test object.

The plans and specifications for an ordinary dwelling submitted to two such groups would be an excellent test of their abilities to interpret difficult subject-matter. If both specifications and plans were submitted I think the Latin group might come off best.

Finally test a group of pupils who do not study German on learning German words. Then divide them according to their success in this and let one group study Latin for three to six weeks, the other history. Then test both groups on German words again. The experiment should also be done with one group actually studying German, the other Latin. Only after such experiments shall we *know* whether we have one memory for Latin words and a totally different one for German words.

If one will be careful to get equal groups equal in rank, as shown by Winch's procedure, and to eliminate or equalize other influences, it would be possible, I think, finally to tell what influence the moderate exercise of one mental power has on others and what influence its extreme and prolonged exercise may have. For moderate and excessive exercise seem at present to have exactly opposite effects.

For this reason and because the subject becomes practiced in the test material, the amount of practice must be carefully determined for each experiment.

However, all this is simply recommending Mr. Winch's method. Beginning with that, take at least twice as much time in devising an adaptation of it as you expect the entire experiment to require. Careless methods have produced most of the errors in experimental psychology.